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EPRI
Electric Power
Research Institute

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U.S. Nuclear Regulatory Commission
MS 1130 SS
Washington, DC 20555

Dear Dr. Silberberg:

This letter offers comments on the draft of BMI-2104, "Radionuclide Release Under LWR Specific Accident Conditions (preliminary), Vol. II: A BWR Analysis."

(1) We have been previously involved in peer reviews of NUREG-0772, Vol. I of NUREG-0956 in addition to the current review. After considering these three reviews, it seems that the resources being applied to the effort are not adequate to match the technical complexity of the task as it has been defined. This is particularly true since the effort correctly requires high quality work in analyzing complicated physical phenomena and thorough visibility of all the steps in the analysis. The latter factor especially requires special effort in writing and communication.

Whether the significant effort required to address the problem as it has been defined is commensurate with the low probability of serious degraded core events is a matter which should be evaluated from time to time.

(2) The analyses are based on the results of calculations using a variety of codes. The descriptions of the codes up to this point have been generally qualitative, and some of these descriptions are not even available so far. Therefore, the peer review on May 24 and 25 was very superficial. It is noted that Oak Ridge has been assigned the task of preparing reports on "code validation and data base status" for ORIGEN, MARCH, MERGE, CORSOR, TRAP-MELT, CORCON, VENESA, SPARC, ICE CONDENSER MODEL and NAUA-4. The proposed scopes of these ten reports is impressive; if the reports are executed in a timely and thorough manner, they will be of great help in future peer reviews. It is further gratifying to note that BCL is undertaking a sensitivity analysis for key parameters.

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Since these codes are often specialized, it would be helpful if independent peer reviews were conducted on each code by appropriate specialists so that their reports are available to the current peer review group as soon as possible.

It is of paramount importance for the codes to be presented in such a fashion that the physical principles are visible and adequately tested so that the reactor safety community has confidence in the results.

We feel that a later peer review of BMI-2104 is necessary when the methodology is better described.

(3) As pointed out previously, there is a paucity of information on the codes. However, in the review of the part of BMI-2104 available to us, several particularly significant points deserve mention. These are the following.

(a) The codes must be used, and if necessary modified, to predict the location, physical state, temperature, etc. of the water in the primary coolant system, the drywell, the wetwell, and containment at various times in the accident sequence. Water is by far the constituent in greatest amount and can dissolve salts, wash down walls, etc.

(b) Concern has been expressed concerning the application of MARCH to a BWR since BWRs contain channel boxes (shrouds). We understand that the BCL group certainly recognizes this problem, but the way in which the problem was approached has not been clearly described.

(c) The core slumping model (page 5-5) directly affects the source term since each part of the core that slumps is added to the source in the primary system. Since the slumping depends on the size of the node, the results are user dependent. Therefore, one part of the parametric study should show how sensitive are the results to the selection of the nodes. In addition, core slumping is described in the CORSOR description rather than MARCH. If we assume that slumping is determined by MARCH, it appears from Table 6.1 (p. 6-37) that the BOIL model A was used for meltdown -- and model A of MARCH is for instantaneous core slumping when

75% of the core is molten. Thus, there seems to be an inconsistency between the slumping model description (p. 5-5) and the actual input to the model (p. 6-37). Also (p. 6-37) "core slumping starts when lowest node in region is molten" -- what if the upper nodes are molten first and freeze later. The slumping model also affects the timing of event.

(d) We believe that SPARC may give low decontamination factors when the aerosol passes through a hot pool. A SPARC vs. SUPRA vs. experimental data comparison should be undertaken.

(4) The Te releases proposed by BMI-2104 are higher in some cases than WASH-1400. The experimental evidence for the release of Te from core-concrete melt is essentially non-existent and should be promptly studied in an experimental program. The rate of release of a volatile material from a large molten mass will be very sensitive to scale. It is not explained how this matter was treated in VANESA. Additionally, the rate of release of Te from the core-concrete melt will depend strongly on the rate of sparging. This in turn depends on type of concrete used. In this report, limestone which will release CO_2 was said to be assumed to be present. The type of concrete should be confirmed. Surry was found to not have limestone in its concrete and this may indeed be true for Peach Bottom.

(5) Some of the statements (for example page 7-28) indicate that the recent BCL-EPRI scrubbing results in which it is shown the aerosol scrubbing by a hot pool is as effective as by a cold pool have not been factored into BMI-2104.

(6) It is not clear to us why a containment dry well break area of 10 ft^2 was assumed, which overwhelms the capacity of the stand by gas treatment system (SGTS). A smaller leak from the drywell could be handled by the SGTS, thereby not putting the reactor building under pressure and damaging its integrity.

(7) Venting of wetwell through the SGTS at a deliberate rate may delay or preclude the failure of the drywell. We believe such a possibility is allowed in the emergency procedures for the Mark I BWRs.

(8) The temperatures shown in the report for the core and the BWR upper internals are much higher than those obtained by us. We have not been able to discover the source of the discrepancy so far.

(9) The core slumping mode assumed in the analysis of the progression of the BWR core degradation appears to be different than that assumed for the Surry core degradation, and it may be the reason for the "high" temperatures calculated in the BWR system.

Could this be explained and the effects of different assumptions on core slumping delineated?

(10) We were troubled by the very large sensitivity (several hundred degrees) of the primary system temperatures to the nodalization of the Mark III primary and containment system. This should be explored further.

(11) Hydrogen combustion loads are supposed to fail the containment in the Mark III TQUV sequency. This appears to neglect the information from recent experiments which show that the hydrogen combustion will be in the form of diffusion flames, which will not generate high pressures.

EPRI appreciates the opportunity to review BMI-2104. It is recognized that the evaluation of degraded core accidents as the methodology is now being developed is very complicated. BMI-2104 needs much more explanatory material before it can be subjected to a good peer review. We realize that the comments just presented are general, but in depth comments cannot be offered until the greater detail is presented. EPRI also wishes to acknowledge the very significant effort on the part of dedicated people which has already been expended.

Sincerely yours,

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cc: J.J. Taylor
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